RESEARCH MANAGEMENT

A PUBLICATION OF THE
INDUSTRIAL RESEARCH INSTITUTE

Volume III, 1960

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FROM THE PRESIDENT OF INDUSTRIAL RESEARCH INSTITUTE, INC.

With this issue *Research Management* reaches another milestone, inaugurating as it does its third year of publication. It has been gratifying to me and to all in the Institute to watch its steady growth in stature as a specialized management journal. We believe we can look forward with confidence to its continued success and further growth in its chosen field.

Under this circumstance, it seems that the time has come to broaden the editorship of the Journal and form a Board of Editors. It becomes an increasing imposition on the time of Mr. Charles Burrill who has served as Editor on a volunteer basis since publication began. I am pleased to announce, therefore, formation of a Board of Editors including, along with Mr. Charles Burrill, Dr. Herschel H. Cudd, Dr. William H. Lycan, and Dr. Thomas H. Vaughn—the latter three drawn from the original membership of the Editorial Advisory Board. Serving with them as Secretary of the Board of Editors will be Mr. Louis Marshall, a fulltime employee of the Institute and a man with broad editorial experience.

It is expected that the policies under which *Research Management* has been successfully launched will be continued, with the increasing load of review and editing shared by the new Board. Papers submitted for publication should be submitted henceforth to the Secretary, Board of Editors, in the offices of the Industrial Research Institute, where they can be handled more expeditiously and with lesser imposition on volunteer personnel. It is hopefully anticipated that in due course the journal may be expanded and more high quality papers accepted. Thus, it can be expected to become even more useful to Research Managers and to those with whom they work.

ROBERT W. CAIRNS

ABOUT THIS ISSUE

In this issue are two papers adapted from presentations at former meetings of the Institute. The first of these is on the ever-present and always controversial problems of effective personnel appraisal; it quickly departs from the conventional approaches of professional personnel techniques and finally arrives at some worthwhile advice for research managers. The author, Mr. A. F. Kaulakis, as Director of the Process Research Division of Esso Research and Engineering Corporation, has had ample opportunity during his more than twenty years in one of the world's largest research and development organizations to develop the thesis he presents.

A second paper presents new thinking in relation to the basic resources on which research must draw and makes suggestions for their expansion. Particular emphasis is placed on ways and means to make them more effectively available to the laboratory scientist. The author is Dr. Raoul J. Freeman, Project Director for the General Analysis Corporation of Los Angeles. Dr. Freeman has written widely on problems of research management and, among other distinctions, serves on the faculty of the Graduate School of Business Administration at U. C. L. A.

Another paper presented at the Fall, 1958, meeting of the Industrial Research Institute serves as a fascinating case history of a successful research project carried out under unusual handicaps and in the glaring light of critical public interest. Few development projects have been the subject of such unusual interest as that of the commercial process for production of poliomyelitis vaccine. The authors are Dr. F. D. Stimpert, I. W. McLean, Jr., and B. I. Wilner, whose baptism in the fire of public emotion aroused by this research can be sensed in their factual account of the work.

A change of pace is provided by Dr. E. M. Kipp of the Sun Oil Company, who makes some pertinent suggestions for better introduction of the newly graduated scientist to industrial research. This is one of those subjects which receives much pious approbation in nearly every industrial research laboratory but about which little is effectively done in most of them. We believe Dr. Kipp's article may induce some of our somnolent colleagues to take a look at their past practices with new recruits when this year's crop comes hopefully in this coming June.

We are fortunate to be able to present a most thoughtful analysis by Harold Gershinowitz on the problems of sustaining creativity in our modern industrial laboratory environment in those all too rare instances where true creativity exists. This paper is adapted from a presentation at a conference on Creativity in an Organizational Environment held at Harvard University this past summer. Dr. Gershinowitz, President of the Shell Development Company, has long been an articulate and effective advocate of all possible freedom for scientific thought and action in our modern industrial environment. We are glad that in this paper he concludes that creativity can be sustained without relinquishing the responsibilities of research management.

If all of the qualities desired in a research scientist were enumerated in the order of importance, the list would be long indeed, but at the head is likely to be found one of two words: creativity or motivation. Surely if one appeared at the top, the other would be second. Our sixth article is therefore of intrinsic importance since it deals specifically with the motivation of the industrial research scientists. The author is Horace A. Secrist, who is Director of Research of The Kendall Company of Boston, Mass.

THE BOTTLENECK IN EFFECTIVE PERSONNEL APPRAISAL: IDENTIFYING WHAT IS LIMITING AND WHY

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Personnel appraisals are generally made for one of three purposes, namely, salary administration, gauging long-range potential, and improving absolute research performance. The design of the appraisal system must keep the end use in mind because the elements that must be built in depend on the end use. For salary administration, the primary need is to establish relative performance; it is not difficult to design a system that is adequate for this purpose. For gauging long-range potentials, observations of current performance are not as important as the exercise of judgment in estimating how a man will probably perform in a situation whose components and dimensions are far different from those in which he has performed; thus, an appraisal system for this purpose relies heavily on observations of general personnel characteristics, that is, traits, strengths, weaknesses, etc. The appraisal techniques for both of these purposes must be adequately discriminatory;

* Presented at the Fall Meeting of the Industrial Research Institute at Lake Placid, New York, October 12–14, 1959.

but the requirements are far more demanding for a system which is intended as a tool in raising the absolute performance levels of our scientists.

A system whose end use is the improvement of research requires an approach which will allow us to identify explicitly that factor which limits the execution of a research problem. If this limiting step can be identified, we have a good basis for establishing why this limitation exists and how it can be overcome. Once we have identified the bottleneck, we are in a good position to determine how it can be broken.

I am not aware of any system which is capable of doing this type of an appraisal job. However, I think that one can be developed. I believe the way to obtain such a system lies in looking at how a research problem is attacked, and, at this point, in looking at the research man. The probability of developing a workable technique with this approach depends on whether there is a rather definite pattern in the way a research job should be carried out. In other words, a common denominator must be found to make this approach workable.

The Common Denominator: A Logical Method of Thought

Let us look at a research project to see whether we can distinguish any patterns. The simplest and least controversial picture of what we see could be represented as a "black box" into which problems or questions are fed, and out of which come solutions or answers. The black box is reducing problems to solutions; it is converting questions to answers; and, as an important side reaction, it is uncovering the unexpected. To simplify our considerations, let us assume that this black box remains in one given environment so that we can rule out the very real but complicating factors of motivation and environment. What we observe by further watching the process pictured is that both the quality and the quantity of the answers coming out of the black box in a given

period of time vary considerably. This black box is the research man.

Now, let us open up this black box and ascertain what steps are going on inside. If we know what steps are involved, we are in a position to determine why certain black boxes produce better answers or more answers than other black boxes. There is an analogy here to a chemical reaction or even to a computer. In a chemical reaction you have to define the mechanism before you can study the kinetics, that is, the rates of the individual reactions involved in the overall reaction. When the mechanism and the rates have been established, that step which limits the rate of conversion of starting materials to the desired products, i.e., the slowest step, can be identified.

An analysis of how our most productive black boxes work reveals a consistent picture. What goes on inside the black box box can be represented in a highly simplified form as the following six principal steps (such other important components as feedback, recycle, or sources from which knowledge is drawn are not included):

(1) Translating or unscrambling the statement of the problem into clear, concise research questions.
(2) Hypothesizing, that is, postulating tentative answers, in order to develop a basis for intelligent action.
(3) Validating the working hypothesis by critical experiments.
(4) Designing and conducting the experimental program required to generate the information needed to quantitize the hypothesis adequately.
(5) Organizing and interpreting the results in a form that is useful for application work.
(6) Applying the results to answer the specific problem.

What I have described is nothing new, although it seems to be rediscovered periodically. It is simply an orderly method of

applying the logic of effective thinking to the solution of some type of problem. This procedure is called the Scientific Method. I am sure that, when you look at how your better research people work, you will have no difficulty in seeing that their approach to problems follows the method outlined.

Researcher Types Exist Because of Limitations

The kind of man that we would all like to have, ideally, is thus rather obvious. He is the man who can handle each of the steps in the research process equally well. Unfortunately, the facts of life are such that there are relatively few men around who are so talented or capable; everybody is limited somewhere. You will find many people whose strengths are predominantly in two or three of the six steps in the research process. They can be very valuable component parts in industrial research. Here is a situation which we should face realistically.

Reaching into our experience with research people, we can easily identify four "types" that are consistent with our picture of the research process. These four types are the theorists, the experimentalists, the data organizers and interpreters, and the model builders. These terms are probably self-explanatory, but I might explain that the model builder is a data organizer who relates the data to theory. I am not referring to the builder of mechanical models or prototypes for experimental use. If we have indeed "typed" these men properly, we have taken into account limitations in their research performance revealed by observations of how well they have carried out their past research assignments.

How serious these limitations are depends on how a man is used. A poor experimenter can be an outstanding theorist without serious consequences to the job if the experimental part of the job is not critical or if someone else does the experimental work. In industrial research, where the team approach to research

projects is often desirable, the effect of such limitations in individuals can frequently be minimized. There are often projects which are so organized functionally as to permit division of the work into the same categories as our men, i.e., theoretical, experimental, etc. In such cases, the best overall research output will be obtained through a careful selection of personnel to take into account their individual limitations as they affect the research process.

An Example of Putting This Approach to Work

Having this model of the research process puts us in a position to carry out a meaningful appraisal, that is, an appraisal that will answer the three critical questions: what is the limiting factor in successful solution to a problem; why does this limitation exist; and what can be done about it. The best way of describing how such an appraisal would work is by an example such as the following simple hypothetical situation, which is outlined with deliberate emphasis on the research terms and components involved.

Suppose that Smith has been assigned the job of improving substantially the yield of cyclohexane in a process involving the catalytic hydrogenation of liquid benzene. Smith conducts a study of variables in which he observes that increasing the hydrogen pressure and the temperature changes have a small but positive effect. He decides that a more active hydrogenation catalyst is needed, and he devises a standard screening test in which various metal catalysts are studied at given conditions of temperature, pressure, and catalyst surface area. The choice of the catalysts studied is a logical one, made on the basis of literature data for hydrogenation. The screening program is itself done efficiently, Smith having first determined that the rate is first-order with respect to partial pressure of hydrogen; thus only a few points are needed in order to establish the relative activity of a given catalyst. Smith has employed valid and accurate analytical techniques; he has calculated

and experimentally verified that higher yields are thermodynamically possible. His results are these: a combination of higher temperature and pressure and the use of a new palladium catalyst will give a yield somewhat below that desired but much higher than obtained at present. However, the catalyst is too expensive, the pressure too high for practical use, and Smith's objective is not met. What went wrong?

In analyzing Smith's performance, we can legitimately question his approach. Specifically, Smith failed to break his problem down into critical questions, to set up a sound hypothesis, and to test it by critical experiments. Having rightly established that his objective was thermodynamically possible, he failed to ask himself the next important question, namely, "Is the rate controlled by chemical limitations, or by mass-transfer, diffusional effects?" Having formulated this question, he could have attacked it by testing a reasonable hypothesis.

This hypothesis could have taken such a form as this: "The rate is limited by the diffusion of reactants to the catalyst surface, and the chemical rate on the surface is much faster than diffusional effects; therefore, chemical considerations play no limiting role in the process." This is a reasonable hypothesis because it is in line with the temperature-pressure effects observed in his preliminary experiments. This hypothesis could be easily tested by use of standard and fairly simple critical experiments that are well known for testing for diffusion limitations. If these tests proved out his hypothesis, the rest of the program would have been totally different. Smith would not have searched for new and exotic catalysts, but he would have worked on such variables as the surface area and pore volume of the existing catalyst. The entire screening program at "standard" conditions would have been eliminated.

Further analysis of the situation revealed that Smith had no undesirable interference from management on how to conduct his project. He had equipment, no unusual pressures in terms of

time, and good supporting facilities. Thus, the bottleneck in the research project was Smith himself and not management. Specifically, the rate-limiting step was his own inability to establish a sound hypothesis. The answer to "why" might have been inadequate background in the area of kinetics and mechanisms of heterogeneous reactions. Since Smith's basic capabilities were good, the action to be taken would be to give him the opportunity to deepen his knowledge in this critical area. The net result from this appraisal would be the upgrading of Smith as a researcher, and thus the raising of the research level of the organization as a whole.

Required: Appraisers Who Are Good Researchers

Putting this approach to work is not a simple task. The critical element is the appraiser. This should be obvious from the example just given, since a deep appreciation of the technical elements in the job is vital. Required are appraisers who are themselves top-notch researchers who will exert the kind of positive leadership needed to make critiques meaningful and effective and who will not hesitate to take appropriate action on the basis of their diagnosis. On the other hand, these are the kind of men who should be appraising researchers from any point of view, regardless of the system used. An approach based on critiques of the research work done should make it possible for such men to do a better job of appraisal because they are oriented primarily towards research. Their training, their experience, and the nature of their work orients them naturally this way, and not towards what makes people "tick." Therefore, this is a critical requirement that can be readily met in an effective research organization. All that is really needed is an environment that places the conduct of constructive critiques of research on the same level of importance as getting the research itself done.

A Working Hypothesis with Potential for Increasing Productivity

Having advanced the hypothesis that a more effective personnel appraisal system can be built by using an approach that emphasizes looking at the execution of research projects, I find myself in the awkward position of not being able to follow through with a critical experiment that would convince you of its validity. Nevertheless, I hope that this hypothesis will stimulate your own thinking on a problem which is of vital concern to all who have direct responsibilities for maximizing research productivity.

I believe that the hypothesis is sound and that such an approach to appraisal has tremendous potential, not merely as a way of getting better personnel appraisals, but as a tool to increase the productivity of a research organization. It will serve this second function by raising the absolute level of performance of the bulk of the research staff and by making it really possible to place each man where he will be most effective. And finally, I believe that this approach could turn out to be a positive and dynamic force in fighting the serious battle against technical obsolescence.

THE BASIC RESOURCES OF RESEARCH

RAOUL J. FREEMAN
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The basic resources needed for industrial research are scientific manpower, fundamental scientific knowledge, and capital. We shall discuss these resources and their relationship to industrial research in the United States. Several recommendations will be presented for meeting our needs for additional amounts of the first two resources. The availability of capital seems to be only a minor factor limiting the growth of research.

Scientific Manpower

The lack of available qualified personnel is commonly cited as a major factor retarding the R & D activities of companies. This was verified by a survey conducted by the National Science Foundation in which more than half the companies interviewed reported that they were unable to hire enough engineers and scientists to meet their needs.

"The need for additional personnel in the research and development activities of the reporting companies covered a wide range of fields . . . chemical, electrical, mechanical, and aeronautical engineering, chemistry, physics, metallurgy, mathematics, and a number of others. The demand for additional scientists

and engineers also extended to all levels of training and to new graduates as well as to experienced men, although most company officials said they had a greater need for personnel with experience or advanced degrees, than for new graduates with only the bachelor's degree."[1]

"The need for 'quality' in hiring research personnel was repeatedly emphasized. Officials of many companies reiterated that requirements for research scientists and engineers cannot be met because well-qualified, well-trained people are difficult to find."[2]

Writers on the economics of research have also taken notice of the above mentioned shortage. Their thoughts are exemplified by the following statements of Brozen.[3]

"A limitation on the ability of increased research to increase profits is the scarcity of technical manpower. If every concern attempted to increase research expenditure, the short-run effect would be an increase in the cost of research with little increase in research results. For the country as a whole, there is a bottleneck on research. Our supply of technically trained, research-minded personnel is limited and cannot be increased quickly. An attempt by all industries to increase research programs would result primarily in bidding up the price of research personnel in the short-run. In the long-run this would increase the number of people who would train themselves for the attractive renumeration offered by research jobs. But training is a long process; also the number of trainees cannot be expanded rapidly because of the still larger process of providing increased educational facilities. This is one of the more important determinants of the rate of growth of the research industry, i.e., the rate at which appropriately qualified personnel become available. An increase in salaries of research personnel would increase the rate, but the response would not become appreciable for several years."[3]

Let us now examine this shortage to get an idea of its magnitude. For instance, it has been estimated that the demand for engineers will exceed the supply by 27% in 1960.[4] This 27% represents about 60,000 engineers, and the additions to the supply each

year are insufficient to meet the additions to the demand. Furthermore, the shortage is very much worse in some scientific specialties, particularly those on an advanced level. Many studies[5] have been made, and all subscribe to the seriousness of the situation.

How can the shortage be relieved, and who is to pay for the relief? We can relieve the shortage by increasing the productivity of our scientific personnel and by increasing the supply of scientific manpower. Productivity may be increased by scientific management of technical personnel. For instance, "support ratios" should be optimized. (The support ratio is defined as the number of supporting or service personnel per "front-line" scientist.) If this ratio is below the optimal value, the front-line scientists are engaged in menial tasks, the value of which is less than that of alternative scientific tasks they could be doing. Other items which could be looked at are the relationships of growth, age, and size of research laboratories to productivity.[6] We feel that the support ratio is the most fertile area for further study.[7]

The short-run supply of scientific personnel can be increased by (*a*) raising salaries and prestige so as to attract members of fringe industries and discourage scientists and engineers from going into other fields; (*b*) more use of older scientists and women scientists; (*c*) short-run education projects; (*d*) company instruction programs. A comprehensive list of suggestions has been given in the *Interim Report of the Joint Committee on Atomic Energy*.[5]

The long-term supply can be increased by expanding education programs, and by offering higher salaries and increased nonmonetary benefits for scientists. Higher salaries will make the scientific professions more attractive to a capable youth surveying the spectrum of possible occupations. In the same vein, augmenting the glamor and public prestige of the scientific professions will attract more people to these fields. Further, we are in an age where scientists are the backbone of our national defense effort, as stated by Walker:[8] "It is not too strong a statement, I think, to say that research and development constitute the broad base upon

which the entire defense effort rests." Thus, an appeal may also be made to patriotism.

Let us now turn to the increased-education suggestion, and examine our educational system to see how our suggestion can be implemented. In this connection, Wolfle[9] points out a large leak in our educational pipeline: "Every year 150,000 pupils who could have become average or better members of most of the specialized fields graduate from high school but do not enter college. Some of these able students will attain positions of high responsibility, they will contribute as much to society and derive as much personal satisfaction from their work as they would had they attended college. But many will not. Without college education they have little or no opportunity to become teachers, scientists, doctors, lawyers, or social scientists . . . As a group they cannot contribute to society as much without additional education as they could with it."[9]

Wolfle further emphasizes his argument by pointing out that only $^{3}/_{5}$ of the top 5% and only $^{1}/_{2}$ of the top 25% of high-school graduates graduate from college. He also maintains that colleges could absorb more students without appreciable loss in the quality of instruction.

The reasons for the loss of the 150,000 pupils mentioned above are ethnic, geographic, religious, and, most important, socio-economic. They can best be related to two factors: lack of money and lack of desire to go to college.

Given a desire to go to college, lack of money can be combatted by financial aid. The government and private foundations are taking steps in this direction at the present time. Unfortunately, the lack of aid is not the major factor causing the leak in the pipeline. To find the corroding influence, we must go back in our educational system to the elementary and high school levels. It is on these levels that a love for science, a "proper" outlook on the world, a desire for higher education, a blossoming of innate abilities, and a counterattack to "narrow" home education must be

formulated. In order to do this job properly, we need well-qualified teachers.

There is a distinct shortage of well-qualified teachers. U. S. Office of Education figures indicate a shortage of 72,300 teachers on the elementary school level.[10] Furthermore, there are 50,000 others who may be described as substandard. Thus, we can begin the relief of our personnel problem by the better utilization of our present supply of well-qualified teachers and by increasing their number. This calls for an increase in teacher salary and prestige.

This leads us into the problem of who is to pay for the alleviation of our shortage of scientific personnel. Currently, more and more private companies are starting fellowship, tuition-refund, and cooperative programs. The expenditures for these programs can be justified on the grounds of an altruistic wish to contribute to the welfare of the nation or as recruitment expense for exceptional talent. However, the only basis for expecting a direct recruiting advantage is a moral obligation on the part of the fellowship recipients. Private individuals may borrow money to enable them to obtain an education, and may then pay it back out of the high salaries they will derive as scientists. However, this will only be done by people with ability and a desire to be scientists.

Many social benefits would result from the alleviation of the research personnel problem. Economic theory teaches us about the relationship of income to investment and that investment will continue until the rate of return over cost falls equal to the interest rate. Successful research yields investment projects with high rates of return over cost. In order for research to be successful it needs inputs of scientific personnel and scientific knowledge. Thus, we can see that national income is related to the scientific-personnel situation. The exact natures of these relationships are not known, but we feel safe in saying that they are all probably monotonically increasing functions over the range of immediate practical concern (i.e., the partial derivatives are everywhere positive).

Increased education of a portion of the population would lead to a higher national level of intelligence, better social conditions, and a more pleasant life in general. Furthermore, we are becoming more and more reliant on research and development for our national defense. Russia is training a large number of competent scientists. For every offensive measure that is produced by Russian R & D, ours must provide a counter measure in order for us to be secure. According to our present national budget, we value defense very highly. There may be grounds for arguing that more of our defense spending should be used to train scientists and engineers.

From the above discussion it is apparent that there would be social benefit associated with an alleviation of the scientific manpower shortage. Waiting for action by private individuals and private firms in response to wage differentials to bring about the changes required may take an exceedingly long time, and furthermore, a socially optimal solution may differ from that which might finally be brought about by private action. Also, our national defense cannot wait for long-term market changes. Furthermore, the amount of influence these long-term changes might have on other factors in our social environment is uncertain. On the other hand, a program of better teaching would have as its basic purpose instilling in our young people a desire for higher education.

Fundamental Scientific Knowledge

"That all technological progress derives from basic or fundamental research and that scientific discoveries eventually lead to the development of new or improved products and processes is widely recognized in private industry. The need for additional basic research findings upon which to build expanded or more effective applied R & D programs was expressed by officials of a large number of companies. About one fifth of the companies indicated that they had actually experienced problems due to a lack of basic information in the conduct of their R & D programs. Additional companies reported sufficient basic data for their cur-

rent activities, but anticipated problems in the not too distant future."[11]

From the above, we can see that there is a need for more basic research and an increase in the state of scientific knowledge. Brozen indicates that a lack of fundamental knowledge is holding down research and development expenditure in certain industries.

"A limitation on increased research outlay is the state of science. This becomes apparent when we compare the industries that spent the largest percentage of gross receipts on research with those that spend the least. One of the distinct differences between these is the state of science underlying them."[12] For example, the electrical machinery, scientific instrument, and photographic equipment industries spent more than 5% of sales for research, but the primary metals and food products industries, which are not so directly associated with scientific work, spent less than 0.5% of sales for research.[13]

"Companies were reported to be most willing to support basic projects designed to help solve problems encountered, in applied research programs. According to official reports much of the basic research undertaken is of this type. Projects having a less direct relationship to the company's business or requiring long time periods for full utilization of the findings are less likely to get support. Furthermore, as one official explained, since basic research is more speculative than applied and its effect on a company's business not as easily evaluated, the company's financial situation is an especially important consideration in undertaking this type of research. Management is not inclined to support much basic research unless the company is in a strong financial position."[14]

The NSF survey further showed that some company officials felt that basic research should be undertaken as a service to the nation. For the same reason, fellowships and grants to universities were also regarded as desirable. Contributing to the above feeling was the fact that some of the past profits of these companies were to a large degree based on scientific discoveries which were not the

result of their own research effort. Thus they felt an obligation to pay a debt.

Other reasons for private companies to undertake basic research are to keep their scientific staff interested, inspired, and happy, to gain publicity, and to gain prestige in the eyes of the community. Aside from these motivations, there is little reason for a private firm to do basic research, as there is usually no direct monetary reward connected with it.

The basic research and, to a lesser extent, the applied research of a large number of firms could contribute much to the advancement of scientific knowledge. Yet the influence of an individual firm is, on the average, infinitesimal. It is a situation analogous to that in economics of an individual firm in a competitive market. No matter how such a company alters the amount it puts on the market, its influence on the prevailing price is negligible. However, if the whole industry changes the amount it puts on the market (e.g., every firm holds back 50% of its stock), then there would be a marked change in price. Similarly, in the research field, the cessation of basic research in a single firm would not greatly affect the advance of the state of fundamental knowledge. However, such an action by all firms would have have an enormous effect.

Some of the arguments presented above in discussing the social-benefit aspects of an adequate supply of scientific personnel also apply to fundamental scientific knowledge. There exists a relationship between national income, investment, successful research, and fundamental scientific knowledge similar to that of national income, investment, successful research, and scientific personnel. Furthermore, since research is essential to our national defense and fundamental knowledge is essential to research, therefore fundamental knowledge is essential to our national defense. Also, fundamental knowledge makes for a more educated citizen, better social living conditions, and is good in itself.

"If science, therefore, is only to be pursued on account of its utility, there must be something more worth the attention of a

rational being than the use of Reason, or something in man which is superior to his mind; but since this is impossible, science is good both in and for itself, and it requires no extraneous inducements to strive after it for its own account, both as an expression of our inward life, and as an acknowledgment of that which is divine within us. That it also produces the most excellent results in a lower sphere, is a consequence of the harmony of reason which animates everything. It is exactly these results which are called the uses of science; and so comprehended, we may call it the excellence of science seen from a lower point of view."[15]

The need for basic research is further illustrated by the remarks of Conant:[16] "A study of the history of the last hundred years provides arguments most appealing to the hard-boiled citizen who looks at all expenditures with a cautious eye. The record is quite clear: from the labors of those who were interested only in advancing science have come the ideas, the discoveries, the new instruments which have created new industry and transformed old ones. . . Time and time again the applied scientist utilizes the new findings of those investigators working solely to advance . . . The engineer must call on the applied scientist, and sooner or later, if advance in pure science ceases, he will call in vain. For the applied scientist will run out of his most precious fuel—new ideas and new experimental results."

Let us now turn to the question of who is to pay for increases in fundamental scientific knowledge. Aside from the previously mentioned private incentives, there remains only the interest of individuals in science as a good for itself to motivate private financing of increasing fundamental knowledge. If we can speak of an optimal amount of fundamental knowledge at a given time, it seems highly unlikely that private action will bring it about. Thus it would seem, because of the social benefits inherent in an increase of fundamental knowledge, that government financing is called for.

Officials of many companies feel that the government should conduct a wider basic research program. The benefits of such a program would be widespread and not subject to being "commer-

cially classified." It is widely felt that the government is the only source which has the needed funds and incentive to assure the necessary scientific base for a continuing progressive economy.

Basic research by both private industry and the government may be done by means of grants to research institutes and universities. Small contributions from many sources to one of the latter could solve a major problem which no one of the contributors would be able to tackle alone.

The state of scientific knowledge should not be defined in terms of discoveries alone. Its definition should take cognizance of the percentage of people who could use it that know of it. Thus, broad communication of results is an integral part of the advance of science.

At the present time, military-security regulations are holding back a large amount of scientific knowledge. A re-examination of what is really essential to hold secret may release much information and stop the wasted effort of people who are solving problems whose solutions have already been found. Below, we present some suggestions for better communication which may be financed either privately or by the government.

We recommend the establishment of more scientific abstract journals and other media of communication, whereby scientists may find out what their fellow scientists are doing. Large companies could undertake to publish their own journals and publicize the research findings of their personnel. Publication of research results should be encouraged by companies. This encouragement can be in the form of clerical and artistic help and also in the form of monetary rewards. In the same vein, scientists should be encouraged to attend professional meetings and present papers. Guest speakers should be persuaded to visit companies and present talks. Many of the above practices are in current use, but their use could be broadened and intensified.

Adequate library facilities are a "must" for every research organization. We also recommend the hiring of scientist-librarians to watch the literature and to file all items of interest under appro-

priate headings so they may be readily accessible to scientists. We are in favor of developing audio-visual methods of scientific communication. Closed-circuit television, motion pictures, and recordings can be utilized far more for the transmission of scientific information than they are presently. By the employment of the above means, much more information would circulate, and the groups of isolated scientific villages which we have today would coalesce into a single booming scientific metropolis.

Capital

The effect of avalability of capital as a limiting factor has also been discussed by Brozen:[17] "There is a limit to the rate at which a company can absorb the output of a research program. It takes capital to build or remodel facilities, to produce a new or improved product, or to take advantage of new methods of turning out a product. The limited availability of capital may then limit the amount of research a company can usefully undertake."

"This limitation should not be stressed for the individual concern, however. A completed project can be turned into an earning asset by licensing it for use by others as well as by being used by its parent. There is, however, a limit on the total amount of capital available in the economy for building new facilities or for remodeling. If the output of research causes a great increase in the demand for capital, capital costs will go up. The increase in the cost of applying the findings of research will reduce the earnings on research to the point where some projects would become uneconomical. This tends to impose an over-all limit."[17]

While we agree with Bozen's analysis, the rate-of-return figures lead us to suspect that there is room for much research spending before the above limit will begin to take effect.

On an individual-company basis, the NSF found that most companies had sufficient capital for the translation of research findings into production and that lack of capital was not a major deterrent to expansion of their research efforts. If some companies

did not carry all their completed research projects into production this was not because of lack of capital.

In briefly summarizing this discussion, we can say that the factors which tend to limit the expansion of the present U. S. research picture are lack of scientific manpower and lack of scientific knowledge. The availability of capital seems to be a minor factor. We must concentrate our efforts on overcoming these shortages in manpower and knowledge, thereby to reach the bright future to which research can lead us.

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POLIOMYELITIS VACCINE: A STORY OF ITS DEVELOPMENT

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Since the introduction of poliomyelitis vaccine in 1954, about 100 million persons, children and adults throughout the world, have received one or more injections. Everyone is familiar with the experience of "getting shots"—in school, in the armed forces, and from the family physician. But how about the vaccines themselves? What are they? What have they accomplished? How did they get started?

In 1796, the cause of smallpox was unknown, but this fact had been established: dairymaids who contracted the minor ailment, cowpox, through milking did not come down with the far more serious disease, smallpox. Dr. Edward Jenner, the distinguished English physician, conceived the idea of employing this principle more broadly, and in that year he inoculated a boy, James Phipps, with cowpox matter obtained from the hand of a milkmaid, Sarah Nelmes. After a few months, the boy was subjected to smallpox, but did not contract the disease. The term "vaccine," derived from the Latin *vacca* for cow, was adopted to describe the substance used in the inoculation.

The principle discerned by Jenner is that a person develops resistance against a disease when he is inoculated with a preparation containing an attenuated form of the causal agent. In the case of smallpox there exists, as he demonstrated, the naturally attenuated form known as cowpox, which is even today employed for protection against smallpox.

The practice of vaccination gradually came to be adopted in England, the United States, and other countries, but not without bitter and at times effective opposition from segments of the medical profession and many well-meaning laymen.

In the case of other diseases, later investigators were to demonstrate that the disease-producing properties of the causal agents may be artificially attenuated by controlled processing, so that such preparations might be used for immunization against disease. The hazard of unexpected reversal of the artificially attenuated agent to its infectious form, however, encouraged the search for safer immunizing preparations in which not only the disease-producing capacity of the causal agents, but also all of their life processes would be eliminated or inactivated, while still retaining their immunizing properties.

About the year 1890, a century after Jenner's work, a scientist in the Pasteur Institute of France, Emil Roux, discovered that the symptoms of diphtheria were caused by a protein toxin which could be produced by growing the causative bacteria in a flask containing nutrient broth. Subsequent investigations by Roux and his contemporaries, von Behring and Paul Ehrlich, resulted in methods for treating the toxin with formaldehyde and subsequent processing to give a product that was no longer toxic. In addition, these investigators showed that the detoxified bacterial substance produced immunity against the disease on inoculation in animals, an immunity which could be transferred to human patients by injection of serum from the immunized animals. Report of the development of this immunizing serum or antitoxin was made in 1894.

At this stage Parke, Davis & Company entered the story. With the announcement of diphtheria antitoxin, far-sighted individuals were convinced that a new era in medicine was at hand. The company pioneered in applying the discovery to large-scale use by development of methods of production and establishment of standards for processing. Meanwhile, the U. S. Congress passed a law to govern the production and interstate distribution of biologics. Administration of the law was vested in the Hygienic Laboratory, now called the Division of Biologics Standards, National Institutes of Health, and in 1903, Parke, Davis & Company was granted U. S. Biological License No. 1 for production of biologicals. This license is still in effect, and in the years which followed, we participated in the research and production of many vaccines and antiserums. In fact, it was at this same time that the first building explicitly for industrial research was erected by the Company at its home plant in Detroit. We had established a biological laboratory earlier and also had introduced the principle of physiological standardization.

These steps showed the thinking and pioneering spirit of management toward research as well as toward recognition and acceptance of the calculated risk inherent in developing the field of biologics. Realizing that injectable preparations made from disease-producing organisms would be vulnerable to many hazards of contamination and variabilities, much thought and effort was given to technical procedures and testing. Consequently, the first organized systematic method of subjecting medicinal agents to clinical proof before marketing them was put into practice with the creation of a Department of Experimental Medicine.

In addition to producing improved versions of smallpox and rabies virus vaccines, Parke, Davis began in 1940 an expanded active research program in the field of other antiviral vaccines, thus placing the Company in a position to take an active part in the development and production of typhus and influenza vaccines in crash projects during World War II. After settling down from the war demands and with the experience gained, a broader re-

search program on cultivation, purification, and inactivation of viruses was established. Because viruses are different from bacteria, the approach to any new virus vaccine led to many intricate problems, and this was particularly true of poliomyelitis vaccine. Bacteria can be grown in large quantities on liquid and solid diets prepared in the kitchen, but viruses require living cells as hosts if they are to reproduce themselves. Rabies vaccine was made from the brains of rabbits inoculated with the virus; influenza, from the embryo chick of fertile eggs to which the virus was adapted. But we knew that poliomyelitis vaccine required something different.

Ten years earlier, during the 1930's, attempts were made to prepare experimental vaccines from the brain and spinal cord of monkeys. Two independent researchers carried out laboratory and animal tests on these separate vaccine preparations which differed in the method of inactivation (or killing) of the polio virus in the nerve tissue suspension—one by formaldehyde, the other ricinoleic acid. Although routine animal tests appeared to indicate these treatments would eliminate the virus virulence and permit larger clinical trials in man, it was soon found by the appearance of definite poliomyelitis among the vaccinated groups that inactivation had not been achieved, and all activity stopped. The tragic incident was publicized in scientific journals and the newspapers, and the impact of adverse public opinion retarded progress in the field for some years. The difficulties of inactivating polio virus produced in brain material, moreover, suggested that this approach to development of a vaccine was not practical.

In 1949, an important stimulus to poliomyelitis vaccine studies was provided by the report of a new approach to growth of poliovirus. A research group headed by Dr. John Enders at Harvard University described the successful cultivation of the virus in test-tube cultures of living cells obtained from monkey kidneys. Research on experimental poliomyelitis vaccines prepared from tissue cultures was begun immediately.

In view of Parke, Davis' background of research, development and production of biological products, the firm was asked in the

early 1950's to cooperate with the National Foundation for Infantile Paralysis in the development of a vaccine against poliomyelitis. A contractual relationship was accordingly established for the development, production, standardization, and distribution of a vaccine. The agreement, involving as it did a pharmaceutical manufacturer, a lay organization, and a medical product, was a radical departure from the usual mode of operation in the pharmaceutical industry. Success could only be attributed to clear understanding, smooth communication, and full cooperation beginning with management policy decisions, then through Research, Production, Control, and other areas to the final distribution.

Early in 1953, Jonas Salk presented the results of his investigations on an experimental vaccine and an outline of a formaldehyde inactivation process which, on the basis of hypothetical inactivation kinetics, was believed capable of producing a safe and effective poliomyelitis vaccine. Plans were made for an extensive field trial of this vaccine by the National Foundation for Infantile Paralysis, the bulk virus to be prepared at Connaught Laboratories in Toronto, and testing and final processing to be done at Parke, Davis. Consequently, all research toward developing our own poliomyelitis vaccine was suspended, and the virus research staff with all their facilities were applied to developing and scaling up the Salk procedure. Because of the desire to carry out immunizations before the spring (1954) season of poliomyelitis would begin, the urge of all interests—the public, the Government, the pharmaceutical industry, the National Foundation—was to complete production of the test vaccine as quickly as was practical. The expected production snags occurred but were overcome on an empirical basis, and processing continued.

During the adaptation of the originally described laboratory process to large-scale manufacturing operations, we obtained extensive experimental data which cast doubt on the original hypothesis of the kinetics of polio virus inactivation with formaldehyde. Meanwhile, however, the field trials of the vaccine were accomplished, and on April 12, 1955, the anniversary of Franklin

D. Roosevelt's death, the Poliomyelitis Vaccine Evaluation Center at the University of Michigan made the formal announcement that the vaccine trials were completely successful. Immediately, the vaccine became a licensed product under the United States Public Health Service regulations. Pharmaceutical manufacturers were given releases of limited supplies of vaccine produced under the tentative minimum requirements.

One month later, it was evident that the experiences of the early 1930's had been repeated. A number of cases of poliomyelitis were reported to be associated with administration of the vaccine prepared by one of the licensed companies. The Public Health Service embargoed all stocks and commenced a complete investigation. Obviously the impact from this situation was great on the industry, the Government, and the public.

Meetings were held of *ad hoc* committees, composed of virus experts representing the Government, industry, and research institutes; shortly thereafter, a permanent body known as the Technical Committee on Poliomyelitis Vaccine was formed to advise the Public Health Service on standards for the vaccine. Certain changes and revisions were eventually made in the recommended production and testing procedures, based largely on analysis of past manufacturing experiences with the original method. The changes consisted essentially of a more rigorous filtration of the virus suspensions during formaldehyde treatment for inactivation, larger samples for safety-testing during the stages of processing, and a test of the finished product in cortisone-treated monkeys as a final check for complete inactivation of the virus.

When the adverse effects of the originally approved vaccine first became apparent, the management of Parke, Davis made the considered decision to discontinue production by the existing method and to concentrate its efforts toward development of a more reliable process. The research on polio virus and its properties begun in 1950 now became of great value. The new research program led to the development of a process which yielded a con-

sistently safe and potent product and one which complied with all of the new regulations.

The major steps in the process which had seemed to require reconsideration and further development were growth of the virus itself, purification of the crude virus suspension, inactivation or "killing" of the infectious agent, and finally, the test for active virus in the final vaccine.

Early preparation of poliomyelitis vaccine had employed rocking bottles of monkey kidney tissue fragments for propagation of the virus. An alternate method, this one utilizing monkey kidney cells dispersed by the enzyme trypsin as monolayers in 32-ounce prescription bottles, was adapted to large-scale use for virus growth. The advantages of this procedure over the early method were found to be (*1*) greater virus yields, (*2*) lower extraneous protein content of the fluids, (*3*) better control of strange viruses derived from the tissue source, and (*4*) greater flexibility in controlling production schedules.

Some type of filtration is common practice in all biological production to insure the greatest purity, quality, and appearance of the product. The crude polio virus suspension naturally contains tissue culture debris, and, although it is less in the trypsinized cell cultures than in suspensions from tissue fragments, the debris is more finely dispersed and tends to plug the asbestos pads usually employed. Initially, this problem was overcome by changing to fritted glass filter candles, but later a more satisfactory Seitz filtration procedure was utilized under proper conditions.

In the original poliomyelitis vaccine process, formaldehyde was employed as the inactivating agent as it had been in well-established procedures for production of other vaccines. The kinetics of the formaldehyde reaction are very complex, however, and not yet completely understood. The data acquired during our investigations indicated that the action of formaldehyde on the virus was not in accord with the hypothesis involved in the original process.

According to the original procedure, the time required for complete inactivation of the virus by formaldehyde could be estimated by measurements of the rate of inactivation during the first 72–96 hours. The data so obtained could then be extrapolated to determine the total time needed to complete the inactivation. However, this method proved unreliable, since the minimum inactivation time determined for a particular lot was found to vary, depending upon the period of observation of the test cultures. The inactivation curve appears to tail off because the virus becomes masked and, though containing the spark of life, does not grow in tissue culture as rapidly as unaltered new virus.

Our investigations proved that a prolonged observation period of the tissue culture safety tests was imperative, for such a test is the only valid control of an inactivation process. An extension of this test to a total of 35 days was studied and instituted in all early experiments as well as for all production lots to provide improved sensitivity.

There were additional reasons for suspecting the original hypothesis of inactivation. The suspension in which the virus is grown contains some of the original medium constituents, by-products of virus growth, and cell protein, all competing for the available formaldehyde. Further, the suspensions are not of uniformly constant composition, nor are other conditions always ideal. Therefore, the reactions between virus and formaldehyde alone do not take place with a high degree of reproducibility and predictability.

Concurrently with the detailed study of formaldehyde inactivation, the application of ultraviolet irradiation as a method of inactivation of polio virus was investigated. Successful inactivation of rabies and influenza vaccine by ultraviolet irradiation had provided safe, stable, and potent vaccines at Parke, Davis for many years. For the studies on polio virus inactivation, a special instrument was developed called the Michael Reese Centri-Filmer. This irradiation unit was developed by Oppenheimer and Levin-

son of the Michael Reese Research Foundation in cooperation with our research staff members. The instruments themselves were engineered by the General Motors Special Problems Department. The main advantage of this instrument, in addition to its flexibility and adaptability to large-volume work, is its ability to form and maintain extremely thin films thus allowing uniform exposure.

The primary component of the Filmer is a vertical, 15-inch, stainless steel bowl which rotates at a fixed speed (1750 rpm). The inner walls of the bowl are inclined outward at a 1° angle so that fluid fed onto the bottom of the rotating bowl is centrifugally spread into an extremely thin film as it flows upward due to centrifugal force. In operation, six ultraviolet lamps (General Electric G18T6) in a special water-cooled holder (30°C.) are suspended inside the rotating bowl so that approximately a 750-cm.^{2} area of the inside surface of the bowl with its flowing thin film of fluid receives incident ultraviolet energy of controllable intensity. The effective incident ultraviolet energy produced by the lamps can be varied from approximately 5 watts to over 30 watts by varying the input voltage and current to the lamps. This, in turn, can be combined with different flow rates of the fluid being irradiated to provide various degrees of ultraviolet exposure.

It should be noted that in all probability the mechanism of inactivation by ultraviolet involves alteration in some degree of the organization within the virus particle and is quite different from the mechanism of formaldehyde inactivation. The results of our research program showed that a combination of these two inactivation methods, one chemical, the other physical, and each differing in its mode of action, resulted in the achievement of a synergistic effect which yielded a safe product without loss of immunizing potency. Since October, 1955, all Parke, Davis & Company poliomyelitis vaccine has been prepared by the combination inactivation process.

Since we have considered in detail some of the steps in poliomyelitis vaccine manufacture, it might be well to review briefly the production method presently employed.

Monkeys flown in special cargo planes from India provide the basic material for growing the virus and serve as test animals for evaluation of the potency and safety of the final product. A period of health examination and improvement follows, during which the monkeys receive a specially balanced diet and antibiotics to eliminate any bacterial infections. The animals are also x-rayed for signs of tuberculosis.

When they have been given a clean bill of health, the monkeys are anesthetized and their kidneys removed to provide the raw material of poliomyelitis vaccine production. The soft internal tissue of the kidneys is cut into tiny pieces and ground with a dilute enzyme solution which digests the dead cells and intercellular material and leaves a suspension of live cells.

The cells are washed and resuspended in a nutrient medium containing vitamins, amino acids, and calf serum as an additional growth factor.

The suspension of live monkey kidney cells, diluted to contain seventy thousand cells per cubic centimeter, is dispensed into sterile glass prescription bottles. The stoppered bottles are placed on their flat side in a warm room maintained at a constant temperature of 37°C., human body temperature.

The cells settle to the bottom inner surface of the bottle, attach themselves to the glass, and start to multiply. After seven days of incubation they have formed a thin layer covering the flat bottom side of the bottle.

The nutrient medium, now containing waste products from the growing cells, is removed from the bottle cultures, leaving the cell sheet attached to the glass. The cell sheet is washed with fresh medium, and balanced salt solution to which has been added a small amount of live polio virus is dispensed into the bottles.

The cultures are incubated at 37°C. for another three days, during which time the virus infects the layer of cells, eventually killing them and producing more of itself. In this way, the concentration of virus particles in the fluid may increase one thousand

to ten thousand times over the number originally inoculated into the cultures.

When the effect of the virus on the cells is complete, the crude infected culture fluid is removed from the bottles and purified by passage through a series of three asbestos pad filters. Samples of the filtered fluid are tested for their concentration of polio virus and are inoculated into guinea pigs and mice to ensure that they are free of contaminating tubercle bacilli or viruses from the original monkeys.

As the first stage of virus inactivation, formaldehyde is added to the purified virus suspension which is then incubated at 37°C. for four days. The treated suspension is then irradiated with ultraviolet light by passage through the Centri-Filmer as the second stage of virus inactivation.

The inactivated suspension is filtered to remove any clumps of virus which may have formed, and samples of the fluid are tested in cultures of monkey kidney cells to make certain that no live virus remains. The suspension is incubated for three days at 37°C. as an additional safety measure, and the test for live virus is repeated.

Since three types of polio virus are included in the vaccine, the procedures described for growth and inactivation of the virus are actually repeated three times for each lot of vaccine. The three separately prepared vaccines are combined, and the tissue culture safety test repeated to check once again for freedom from live virus.

A preservative is added to the vaccine to prevent subsequent contamination.

The potency of the vaccine is determined by inoculating monkeys and, after a suitable interval, measuring the level of antibodies developed by the animals in response to the inoculation.

The tested vaccine is filled in vials, but before shipment to the pharmacist, the product must undergo another series of tests. Samples of the vaccine are checked once again for freedom from live virus, this time an elaborate test in monkeys. Additional tests of purity are also run.

Each lot of poliomyelitis vaccine is subjected to a series of 43 separate tests varying in length from a few days to more than a month. The procedure of manufacture and testing is indeed long, but production of poliomyelitis vaccine is the manufacture of protection against crippling disease. We can permit ourselves no room for error.

This brief review has served, I hope, to demonstrate the contribution the modern industrial organization may make to the development of products for the prevention of disease and for the maintenance of man's good health and welfare. The effects of the coordinated effort of research, production and marketing are graphically shown by the following: I mentioned earlier that 100 million people had received injections of the poliomyelitis vaccine. This occurred in a period of about five years from the time that laboratory development of the vaccine was first announced. In contrast, so extensive an application of the smallpox vaccine required the lapse of 150 years; while in the case of diphtheria antitoxin, which was first disclosed during the early stages of transition from individual research to well-organized group effort, the time needed was about 60 years. One cannot help but reflect that were the resources of the modern drug industry available at the time of the discoveries in smallpox and diphtheria, both of which were quite as serious as poliomyelitis, and far more prevalent, the saving of life would have numbered in the millions.

The contribution of biological industry through the development of poliomyelitis vaccine as a safe, potent, and practical commercial product by coordinated efforts was an almost fantastic operation with an amazing outcome. There has never before been such a project of universal interest which involved research, production, sales, and management of industry, several Government agencies, university laboratories, a publicly subscribed lay organization, and the world press in the successful trial and establishment of a human biological product.

INTRODUCTION OF THE NEWLY GRADUATED SCIENTIST TO INDUSTRIAL RESEARCH

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More is written and said than ever before about the training and education of our scientific manpower. It has become painfully clear that the communist world is relying upon its rapidly growing mastery of science and technology to outstrip, indeed to overcome, the free world in the struggle for survival of conflicting ideologies.

As an admitted oversimplification, we seem to be primarily concerned with two aspects of the scientific manpower question—quantity and quality. There are those who point with alarm to the fact that Russia is rapidly increasing her already substantial lead in the number of students graduating from technical courses of study. There are others who minimize the quantity factor, pointing out that the quality of the technological education and the environment in which the scientists are to practice their profession are matters of much greater importance than mere numbers.

I would like in this discussion to consider a third factor which I believe has not been so well publicized. While common to both the quality and quantity aspects of scientific manpower, it is more critical when related to quality. This third factor is the use efficiency whereby the knowledge and training of our scientific

manpower resources are converted into new and useful applications. Specifically, I have in mind the newly graduated scientist who is beginning an industrial research career. The skill with which this introduction to industrial research is achieved in each case will determine to a significant degree the total effective use of our nation's resources in scientific manpower.

The first area to be considered is that of the preparation of the student prior to graduation with respect to those facts of life in industrial research which assume importance along with individual technical competence. The new graduate often approaches his industrial research career with little realistic concept of the nature of industrial research. Particularly, he is generally unaware of those things other than technology itself which may often, in the final analysis, be controlling. It is fortunate that young people are generally more idealistic, optimistic, and uncomplicated than are their older associates. Unless they can develop suitable protective philosophies and skills, they may be subject to disillusionment, confusion, loss of self-confidence and may be less able to make creative and original contributions. Preliminary corrective action could properly originate in the academic institution with the cooperation of industrial research organizations. Apart from industrial employment during vacation, not always available to every student, an important and helpful activity would be the presentation during the senior year and graduate school of a series of discussions on the limitations, opportunities, and challenges of industrial research. Emphasis could be placed on the nontechnical problems which control the climate in which the technical skills of the apprentice scientist will flower or die.

The individual topics comprising the series could be worked out with the counsel of well established industrial research people so as to create a course which is coherent, has definite objectives, and is in effective sequence. The speakers should be selected from sympathetic, qualified, and interested personalities with broad experience in industrial research. The course should create a realistic awareness by the industrial research scientist-to-be of the fact

that his degree and scholastic record are accepted at face value only to the extent that they provide the initial job opportunity. Immediately thereafter, they assume rapidly diminishing importance in the face of an accumulating performance record.

A degree, particularly the doctorate, normally has great prestige in the students' eyes, almost to the point of endowing the possessor thereof with infallibility in his field. It is, therefore, often an unexpected shock to the neophyte Ph.D. when he first learns that, even though he is now Dr. John Doe, there are some people among those who are expected to take action on the basis of his work, judgment, or opinion, who are inclined to take a skeptical attitude, even one of disbelief! Whereas the scientist is trained to get the facts as the basis for action, he soon learns that this is often not enough. Others may and often will dispute the facts, his interpretation of the facts, and the applicability of the facts to a particular problem. Furthermore, in such situations it frequently appears that the question of whether the skeptics have supporting contrary evidence seems to be quite irrelevant! An understanding of the reasons for such situations and what can best be done should be part of the proposed course.

Most of the best science graduates regard themselves as dedicated seekers of the truth. They feel challenged by the opportunity to excel in their research as a means of serving their company, advancing its interests, and, thus, their own careers and status. Accordingly, after having fulfilled an assignment or initiated and completed a project in a particularly successful manner, too often the scientist is demoralized by meeting disfavor instead of approbation and constructive action. One reason is, of course, that as in any other group of individuals, the "part" cannot readily become greater than the whole, and it is indeed possible for a man to do too good a job, if in so doing he is unable to demonstrate tactfulness, modesty, and acknowledgment of the contributions of others. Again, how does the new scientist prepare himself for a healthy and constructive reaction to such situations?

Another problem which could well be explored with the student prior to graduation is the question of salesmanship. He needs to realize that good salesmanship, scientific integrity, and competence are not incompatible. Rather, one may seriously suffer from lack of the other, insofar as effective use of research findings is concerned. This can be of particular significance to the industrial research scientist who is interested in the administrative areas of industrial research.

Still another problem is that of the impact of corporate policies, ways of doing things, communication channels, etc. upon the neophyte scientist who is at the stage of his life where he is properly most eager to accomplish great things. He may soon discover that his earnest and well-intentioned efforts may have earned him the unofficial yet damning title of "boat-rocker." The late "Rocky" Whitmore used frequently to counsel his graduate students at the Pennsylvania State University on this matter, in effect advising that one should not attempt to change corporate or departmental policies until one had become part of the group charged with these responsibilities. This is good, conservative advice unless one likes to live dangerously and has other dependable sources of income. This problem could be constructively discussed at the student level, thus perhaps avoiding later frustrations and disillusionment of a good man.

Another facet in preparing the student for industrial research is acquainting the student with the characteristics of administrative research jobs as opposed to bench research and with the way in which industrial research experience may serve as preparation for other types of work within a company.

Also, the student should be given some guidance in the ways and means of evaluating prospective employers. If it is important for the company to be selective in screening applicants, it is equally important for the individual scientist to evaluate the group to which he is seeking admission, and to do so in the light of his own personality, objectives, and values.

So much for a limited appraisal of some of the matters which could be reviewed to advantage with the prospective industrial research scientist prior to graduation.

Now, what about industry's approach to the prospective new employee both before and after he is employed? First, I would suggest that, after the first culling of potential candidates and before the firm offer of a position is made, more effort and time be devoted to a review with the prospective employee of the several factors which will so largely determine whether the proposed association is to be a long and mutually fruitful one. Specifically, this could start with a description and analysis of corporate, divisional, and departmental organizations and their interrelationships. Granted, some companies insist they have no organization charts and choose to discourage emphasis of this subject. Nevertheless, regardless of official policy, unofficial but *de facto* organization charts do exist, are recognized, and used. Hedging on this subject can cause subsequent confusion and problems for the new employee. Discussion of this area should cover line, staff, and combinations thereof, to whatever extent these are operative. The new candidate should also know to whom his prospective immediate superior reports and should have a generalized understanding of his particular niche in relation to his immediate group, the R & D department, and the company structure as a whole.

The company's definition of Research and Development as it is actually supported and operated should be explained. The R & D program should be briefly summarized as to balance between types of projects and objectives.

Answers should be supplied to such questions on R & D policies as the following: Is the company emphasis upon production trouble-shooting, sales service, product development, process control, quality control, practical applications of published and known technology, or does it provide for a progressive policy in supporting the search for new knowledge within broad as well as specific areas? What is the relative emphasis upon long-term and

short-term research? What of the policies controlling the use of the R & D technological output? Does R & D have an opportunity to participate in reduction to practice, be it in production, sales, or market development, or is R & D expected to remain in careful isolation and shielded from such activities? What does the R & D group expect of management, and to what extent is this expectation fulfilled? What does management expect from the R & D group? How does communication between the R & D group and other company groups and personnel operate? Is the company "committee-oriented" or "individual-oriented"?

Answers should also be provided on questions on corporate policy such as the following: To what extent do corporate policies provide for individual growth and advancement? Are the training programs highly formalized or informal? Is rotation of personnel encouraged as to specific jobs and to other departments? How are individual performances evaluated? Does actual top-management policy require that each and every segment of the company make the maximum possible use of the R & D department and judge them on their ability to do so? Does corporate spending for R & D fluctuate with the ups and down of the annual or quarterly earnings reports? These are matters which, while seemingly obvious, are seldom adequately covered in the pre-employment contacts. (Even more regretfully, these points are sometimes not touched upon for many years afterwards.)

Another problem is the stress and emphasis placed by the recruiting teams of many corporations on the physical and financial assets of their R & D groups. If done to the relative exclusion of fundamental considerations of job challenge, job content, and humanistic factors, a misleading picture can result. Important as are the physical and financial resources, no substitute has yet been discovered for dedicated effort and the ability to react resiliently to failures, setbacks, and disappointments.

I submit that a few extra hours devoted to a realistic review and appraisal of these questions with the prospective employee would pay rich rewards in terms of lessening of subsequent prob-

lems. Our way of life in America is such that we shall never chose to match the Communist world in the training of mere numbers of scientists and engineers; rather, we must seek out and develop better means of identifying quality, and putting this high quality of scientfic national resources to the most effective use possible. We cannot pay the terrible cost of negligent fitting of pegs into mismatched holes.

Finally, the new scientist joins the company. He is now a part of industrial research. Is his future and his value to the company a matter of catch-as-catch-can? This is a question which is in urgent need of greater attention and better skills in many companies if we are to capitalize upon quality in scientific human resources. I do not propose to explore this broad and complex area in anything but the most cursory fashion; nor, indeed, am I competent to do otherwise. Yet, my observations over a period of many years lead me to suggest a few problems which I believe are typical and suggestive of the broader picture. The relative pertinency of these problems and policies and their possible solutions are matters justifying greater consideration.

Each company has its characteristic ways of doing things—its formal and informal house rules. The new scientist too often becomes familiar with these through accidental collision—generally in a dark and unlighted corridor. The resulting mental and/or spiritual black eye is unpleasant. One of the first things, therefore, that should be included in the new employee's indoctrination is a detailed and thorough discussion of the policies and *the reasons for them*. If good reasons cannot be found, the rule should be abandoned or modified. Most policies originate in some problem or need and serve or have served definite ends.

An understanding of the whys and wherefores of the company's procedural and administrative policies would go a long way towards encouraging cooperation and minimizing misunderstandings and resentments. Also, criticisms of these policies by the new employee would more often be of a constructive and positive nature.

Next, the employee should be given a clear concept of how

he will be evaluated as to performance and potential—what people, mechanics, and procedures are involved. Assignments should be outlined clearly: to whom is he directly responsible; the objective; pertinent background; expected action if success is attained. The procedure involved should also be detailed: Is he expected to ask for and receive detailed instructions, or is he free to carry out the project in his own way with responsibility only for end results? Is the individual researcher expected on his own initiative to enlist the advice and assistance of other members of the staff, whether in his immediate group or not? Are written reports expected to conform to certain requirements, both as to method and order of presentation of context as well as physical makeup? Are interim and/or progress reports expected in addition to final reports? Is the author of the report expected to interpret his findings and to propose recommendations for further actions based upon the findings, or does his supervisor or someone else along the line expect to do this? In addition to his specifically assigned projects, is he encouraged to take the initiative in suggesting new projects, or projects which might compete with currently assigned projects, both in his own group and in the R & D organization as a whole? If so, how does company policy assist him in making contributions to the R & D program? If the new man feels that R & D policies and mechanics of doing business can be improved, is there realistic provision whereby he can contribute in this area? If he has something to get off his chest, real or imagined, how can this be done without compromising his status and rating? This last is particularly troublesome when the immediate supervisor has neither the inclination nor the skills to handle this aspect in anything but a negative or defensive manner. I believe much good could be accomplished in this area through the medium of an effective personnel counselor, even for relatively small organizations.

Industry needs to improve its techniques in keeping the research man informed as to his performance and the plans for his future in the light of his past and current performance. Most research men have the maturity to realize that their superiors are not

seers or prophets, and thus cannot promise specific long-range advancements and promotions. They can and should, however, be ready and able to give the employee a reasonable idea of his competitive status for any specific future potential promotion. Furthermore, he should not be required to make the initial approach to find this out.

Another sensitive area for the individual scientist is that of recognition and sense of participation. For example, in meetings which involve projects on which he is working, will he occasionally be invited to attend and be given an opportunity to present his own work? The problem of corporate communications in the broad area of policies, long- and short-range planning, public announcements, promotions, certain company actions to be shortly taken—these require continuing study for improved results in bringing about the greatest possible return from the investment in scientific manpower. We so frequently become involved with the problem of downward communications, that the at least equally important matter of upward communications is neglected.

We cannot afford the luxury of buying quality scientific seed only to plant it in hostile soil in an unfavorable climate. We spend untold millions in the conservation of natural and manufactured resources through studies of corrosion losses and mechanical wear. We spend billions of dollars annually on repair and maintenance costs to ensure the best and longest economical productivity of our factories. Selection, "repair and maintenance," and improved use of our human elements of production are even more worthy of consideration and attention.

In the final analysis the challenge to industrial research management is that of actually providing job situations in which the maximum potential of the research scientist is constantly realized in areas of mutual interests.

Above all, it is essential that what management preaches and what it practices should result in as few discrepancies as possible.

SUSTAINING CREATIVITY AGAINST ORGANIZATIONAL PRESSURES*

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This paper is not a general analysis of the question posed in the title but a resume of personal experience and opinions. It is my conviction that the research laboratories of the Shell companies in the United States, which have a total staff of nearly 3,000, of whom about 1,000 are professional scientists, engineers, and mathematicians, have been rather successful in maintaining environments in which creativity can flourish.

There are eight separate laboratories; three in Shell Development Company, three in Shell Oil Company, and two in Shell Chemical Company. In size they vary from less than 100 to more than 1,500. Their fields of technological responsibility cover the whole range of activity of the Shell companies, the search for crude oil, the efficient recovery from the earth, transportation to refineries, methods of refining, discovery, and development of petroleum products, as well as synthesis of chemicals, not necessarily of petrochemical origin, ranging from solvents through resins and plastics to agricultural pesticides. The academic competence of the staff covers almost all scientific and engineering disciplines.

None of the laboratories has sprung full-grown from the head of whatever god represents corporate management. All have

* Presented at the conference on Creativity in an Organizational Environment at Harvard University, July 22, 1959.

grown gradually. Their organizational complexity has increased both with their own growth and with that of the parent companies.

This description of the nature and structure of our research activities is not made in order to offer them as models but rather because their diversity provides material for case studies. My own experience with research in an organizational setting has been exclusively within Shell. As a person trained in physical science I appreciate the need for a theoretical framework into which observable phenomena can be set. I am distrustful of and impatient with the "practical" plant operator who depends on his own experience and intuition in solving his technical problems and in determining the need for and value of technological innovation. I recognize that the problems of creativity in an organizational environment lie within the realms of psychologists and sociologists, with whose current efforts I am familiar and in sympathy. It is my opinion, however, that an adequate theoretical framework is still lacking and that research management is still very much an art. What will follow is, therefore, a discussion of elements of organization which interfere with creativity and an analysis of techniques to reduce the amount of interference, presented in full realization that these elements are probably much more complex than they seem and that the independent variables not only have not been separated but probably not even properly identified.

In the examination of the effect of organization on creativity it is necessary to consider two kinds of organization: the external organization, which affects the relationship between the research group and the rest of the corporate structure, and the internal organization, which affects the research worker in his day-to-day activity. Since the problems of internal organization are easier to describe than those of external organization, they will be considered first. They are easier to describe, not because they are less difficult or less important than the problems of external organization, but because they can be defined in more conventional terms of administrative practice, personnel policy, and fiscal procedures.

In the first place I want to make it clear that I think that organization is necessary. It is not a necessary evil. It is only in

theology or ethics that an evil can be considered necessary. Organization is necessary for the welfare of the creative worker. Organization is needed to provide the worker with the physical environment, facilities, and material he requires. Organization is also needed to provide the research man with mechanisms for bringing his achievements and requirements to the attention of those who can appreciate them and do something about them.

One hears much discussion about the effect of size on the creativity of a research group. The usual sentiment is that bigness is bad and therefore to be avoided. Begging, for the moment, the question of whether or not bigness is bad for the research worker, one must acknowledge the fact that if a lot of research is needed, a lot of research must be done. Whether the sponsor is big business or big government, its needs make for big research. Somewhere in the organizational relationships, provision must be made for coordinating the research effort and correlating the results of research. It is as impractical to bemoan the need for an administrative hierarchy in research as it is to regret that the town meeting type of government cannot be used to run a city like New York or even Cambridge. Nostalgia is not a fit emotion for a scientist, and the romantic view is not necessarily the most accurate one.

It is by no means certain that the small independent research group is the most effective one. One does find in many a spirit and a liveliness made manifest in an atmosphere stimulating to the observer as well as the inhabitant. It is also possible to preserve such small areas of empathy within a larger unit. Since between areas of empathy there are often zones of antipathy this does not make life easy for the administrator of the whole. This, of course, should be taken into account in determining his proper compensation. But, seriously, bigness is not incompatible with creativity, and there are advantages which size brings in facilities and in availability of special services and expert advice in remote fields which should tend to compensate for the principal disadvantage, which is, in my opinion, the possible loss of group identity of one or a few outstanding individuals, their associates, and disciples. After all, the men in industry who have won Nobel prizes

have come from the largest, not the smallest, research laboratories.

One measure of the productivity of an individual is the number of technical papers he publishes. This measure of productivity should be directly proportional to creativity. Below are some data on the number of papers published plus patents issued per year per professional employee for two of our laboratories. (Patents have been included since they are a form of publication.) One of these laboratories has been growing steadily for ten years, from 40 to 150 professionals. The other is larger and has oscillated between 500 and 600 professionals during the same period.

For the larger laboratory the number of publications plus patents per professional per year has fluctuated around an average of 0.28. For the smaller, growing, laboratory this ratio began below 0.20 and has been rising steadily. During the last three years, since the time when the professional staff passed 120 the ratio has begun to fluctuate around 0.27. One must be careful not to make too many generalizations from these data. The two laboratories are very different from each other in fields of activity. These figures do give some indication that size has not reduced but rather increased productivity per individual. Publication and patent policy are identical for the two laboratories, but since policies vary enormously from one company to another it would be unwise to compare these ratios with those of other laboratories in an attempt to evaluate their relative efficiencies.

Let us now consider the services that are needed by research and required by corporate procedures. It seems to be possible to maintain a loose and informal type of organization until the number of professionals reaches 30 or 40. At that time a departmentalized structure, still fairly loose, becomes necessary. When the professional staff approaches 100, a more formal organization with definite channels of communication and an increase in management levels becomes desirable. The transition from one kind of organization to another is a very difficult one and causes more problems of morale, which strongly influences creativity, than the increase in organization itself, provided, of course, that the organization is one properly designed for a research establishment.

The kind of organization needed for research is in many ways very different from that of an ordinary business enterprise. Both are intended to make more efficient the operations they hold together. The difference lies in what constitutes efficiency. Efficiency in most operations can be measured in terms such as unit costs or output per machine or individual, which involve statistical concepts of average salary, average performance, and the like. Perhaps one should stop with the statement that efficiency in most operations can be measured. Efficiency in research cannot yet be measured. If it could be, it would have to be in terms of individual rather than average performance. Efficiency in personnel administration for research lies in procedures for recognizing outstanding merit and in means for spreading salaries away from an average rather than having them closely clustered. An efficient purchasing and stores department is one that provides the research man with exactly what he asks for at the earliest possible moment even though this may sometimes not permit solicitation of competitive bids or large-scale discount purchases. Financial practice should be primarily for audit rather than control.

Although our general line management consists of career research professionals, we find it necessary and desirable to fill many jobs in personnel, financial, and purchasing-stores departments with career men from these staff departments in other divisions of the corporation. This is mainly due to the fact that we lack enough opportunities entirely within research for really good specialists in those fields. A spell of several years in research is therefore used as part of the training and broadening of such staff. When first transferred to research, the following sequence of reactions is typical for these employees: (*1*) shock; (*2*) attempt to help us see the light and persuade us to follow "company policy"; (*3*) awareness that research operations and research people are different; (*4*) conversion to the research point of view. This process is sometimes called "brainwashing" by their colleagues or superiors in the staff departments.

The pressure to make the service departments more efficient within themselves is always present. Research manage-

ment must be continuously alert in order to make sure that such apparent improvements do not result in hindrance to the progress of the research work or the morale of the research worker. This means that the research administrator must be well aware of the nature of the research process. Our method of achieving this end has been to have, as heads of our research laboratories and supervisors for research workers, professional men who have moved into administration from personal scientific activity. While disorganization is not our prime objective, we tend towards the least amount of order that can be steadily maintained short of chaos.

Pressures from external organization that tend to reduce creativity can be either specific or intangible. The most dangerous is the complete domination of the research program from the outside. The man of affairs, whether he be business executive or military officer, is rarely in a position to know what research can do to help him. He knows very well what his current problems are, but his extrapolation to what his problems will be in five or ten years is seldom accurate.

This particular form of pressure affects applied rather than basic research. During the past ten years such glamour has become attached to basic research that even nontechnical executives are very happy to see done in their laboratories work for which no commercial objectives are claimed, particularly at the fraction of total effort prevalent in most organizations. The glamour itself can, of course, be exploited with commercial benefit in public relations, in advertising, and in the stock market. It is in the realm of applied research rather than basic that control of the research program by the potential user can stifle initiative, ingenuity, and creativity. There is a danger to creativity in basic research that arises from a more subtle organizational pressure which will be taken up a little later.

It is obvious that the research worker is in no better position than the man of affairs to determine unilaterally what the research program should be. What he can contribute is a knowledge of the science or technology currently available which may aid in solving the problems, an opinion of the probability that a solution

can be obtained, and an estimate of the time required for such a solution. He can also guess about the effects that science and technology in related efforts may have on the future existence of the problem as stated. An objective that is stated too rigidly will interfere with creativity. An objective that is too loosely defined will interfere with applicability. The desirability of an assumed objective can change during the progress of a research project, sometimes because of the results of the research itself, and sometimes because of changes in the sphere of potential applicability. It is not easy to develop a mechanism which will permit a continuing review of research progress and practical objectives but it is essential that such a mechanism exist and that it exist in a form which will not inhibit creativity. In our own organization we are continually experimenting with such mechanisms. Their usefulness depends not only on the formal organization but on the individuals involved.

In the end, however, the responsibility for the research program should lie within the research organization. The position of research management in the overall corporate structure should be such as to provide it with enough information about and voice in policy to make its judgments valid and enough status to make its decisions or recommendations stick.

The creative act is essentially a personal and private process. With rare exceptions, however, the creative research worker is very sensitive to his environment. Each research laboratory has a character of its own. This character is determined not only by the research staff but, perhaps even more, by the corporate origins of the group and its place and recognized function in the corporate structure. The industrial research man knows very well that, when properly done and properly used, research can be of great financial value. If he sees that the only use made of research is in improvements that require little risk and little capital investment, he becomes reluctant to direct his creative talents to novel but risky projects. The basic research worker is quick to recognize when he is being used as window dressing. He is also very sensitive to the quality of his administrative superiors. If the man who

is expected to do basic research thinks that the nature and function of basic research are not properly understood, or if he feels that research management is not capable of recognizing outstanding achievements in pure science, he will usually adapt himself to applied research activity or, in a few cases, look for more appreciation elsewhere. On the other hand, in laboratories which are well managed, which have high corporate prestige, even when their primary function may be applied research, good and exciting basic research will be undertaken. These are some of the intangible aspects of organization that can affect the morale of research and particularly can determine the very existence of good basic research.

The modern scientific research laboratory is a novel and bold experiment in the harnessing of creativeness. In the past it has not been unusual for an individual to depend for survival solely on the products of his creative intelligence, but groups of creative individuals have usually had some other function, educational, religious, or cultural, which furnished the basic needs for existence and permitted creativity to be indulged almost as a by-product. The scientific research organization is dependent for its continued existence on the demonstrably creative products of its members. With the increase in size and importance of organized research has come an intensified interest in the phenomenon of creativity, obviously in the hope that a better understanding of the process would make possible first the identification and selection of creative individuals and second the establishment of an environment which would provide these individuals with the maximum stimulation of their creativity.

I am convinced that the success of organized scientific research is due to the fact that the organizations have been built up and are controlled by scientists. Creative men themselves, they have sought to select the right kind of men and to provide them with facilities and environments which in their own experience would be most likely to lead to success. Laboratories vary widely in character but the good ones share the quality of having the organization built to fit the individual research worker. The fit into corporate organization is essential but secondary.

MOTIVATING THE INDUSTRIAL RESEARCH SCIENTIST

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What manner of man is this industrial research scientist whom we desire to motivate? Ask the business executive and you get a picture of the temperamental egghead. Read the recruitment brochures and no Oriental princeling was ever so coddled. Ask the researcher himself—but perhaps better not.

Actually, of course, like people in other walks of life, scientists have a wide range of personality and of family, religious and regional background. They may be extroverts or introspective; jolly or moody; good with people or unapproachable and shy. Despite these normal variations, it is easy to discern common characteristics which distinguish researchers from the rest of the organization. These common traits come, in part, from the nature of the individuals. These are the kind of people who in college chose to concentrate on the study of a scientific field, such as physics, chemistry, or microbiology. Partly these traits come from the habits and training that these disciplines have instilled in the individuals and partly they come from the attitudes which have been acquired by exposure for a number of years to the academic environment.

Like the executive group, these people intrinsically are highly intelligent and logical, but they are, perhaps, more self-conscious about their intelligence. They have more regard for thought *per*

se; their naturally logical natures have been developed by training in the rigorous logic of the scientific method. Also promoted by the method of science is an attitude of skepticism. A consequence of logic and of skepticism is that our research man appears to others to be slow to arrive at conclusions, tending to reserve judgment until a high degree of proof is available. He tends to be impatient with intuitive or emotional approaches to problems. Here, then, is a basis for conflict between the scientist and the man of affairs. Actually, the conflict is needless, for the restrictions of the scientific method would leave the business man powerless to act in daily situations, whereas it is the scientific method which makes a man into a scientist. In other words, each to his own trade, and let him respect the methods which the other has developed wherewith to ply his trade.

A well known and not inconspicuous trait of the research scientist is his individualism. He just naturally is made this way. He may be quite unconscious about it, seemingly unaware of his own lack of conformity and unconcerned with the greater conformity of others, or he may be rather militant about it, showing pride in his own individualism and scorn for the desire to conform which he observes in others. This is a prominent source of friction in the teaming up of industry and the scientist. The scientist—particularly when fresh from the university—is sometimes hypersensitive about rules or restraints which are common in business. On the other hand, the business executive can be exasperated at the peculiarities and independence of a research man. I have no magic formula by means of which to settle the dispute, except to urge mutual tolerance and respect. Let the business man accept the desire for individualism on the part of the research man as unavoidable and, perhaps, even legitimate. Let the scientist not make a fetish of his individualism, and let him acknowledge the problems which are created in industry whenever the research department demands special treatment. I must confess some special sympathy with the researcher. I think that management often does not fully realize how great an adaptation the

scientist does succeed in making when he moves from the university to the industrial laboratory.

The industrial research man feels he is different because he considers himself a professional. He has chosen to devote several years to an educational pursuit which is exacting and time-consuming. He did this because he wanted to be a scientist, not a businessman. Having completed his education, he elected to enter an industrial laboratory rather than to remain with a university. Please notice that I said he chose to enter an industrial laboratory. I did not say he chose to enter industry. He still considers himself a scientist rather than a businessman. Certainly he feels he is a part of the company and he is loyal to it. He knows his fortunes are tied to those of the company; but at the same time he is a member of the scientific community.

When industrial management has sought to add scientific research to its other resources, too often it thought of this as a matter of hiring scientists. A more mature concept is that of creating a research institution within the company. If intentions do not go beyond acquiring a bag of skills to apply immediately and directly to the improvement of products and processes—in other words, to use the man—then the more primitive concept is appropriate. If the intention is broader and more permanent—to produce innovation and long-time growth, to develop the technology of the business—then we should think not of capturing the scientist from his natural habitat, but of building his natural environment into the business. The natural habitat of the scientist is the university. The accomplishments of science have been produced not by isolated individuals, but by the community of scientists—each building on the work of others. Over many hundreds of years, scholars and scientists have been attracted to centers of learning or universities, and have worked out an environment and means of communication which are very well adapted to them and effective for their work.

It is no calumny to say that, frequently, the research scientist is not a well-rounded person. Neither his aptitudes nor his

interests are universal. He is passionately interested in his primary job and very much *not* interested in, or oblivious to, many other matters which arise in daily life. He knows this (although he may not readily admit it) and in order to concentrate on his job, he wants others to protect him from these distractions. Unless we cheerfully accept this as a fact, the business man can become irritated with the "impracticality" of the scientist, and the scientist resentful of the "red tape" and "details" of ordinary business operations.

We have now devoted some time to the nature and mental attitudes of the industrial scientist. Now, what are his main drives and goals? What is he striving for? What does he want to become?

Above all, he wants to produce and he wants to grow. He has two loyalties: he is a company man, and he is a scientist. He wants to produce for the company as a scientist; he wants to grow within the company and in the eyes of the scientific community. Our problem is not merely the elementary one of providing a motivation. Our research man is already most strongly motivated. The challenge is to management, to open up fully to him opportunities to produce and to grow and to reduce to a minimum those things which can frustrate and confuse him and hinder his drive towards these goals. Our concern should be not how to motivate him but how to de-motivate him the least.

What does the business executive want of the research man? He wants him to produce and to grow. Thus, there is no conflict in objectives between the industrial scientist and the business executive. Failure in the push toward the common goal can come from lack of mutual understanding, and from poor communications.

At this point management may say, "Of course we want research to produce, but to produce what?" If management does not know what it wants from research, or if there is misunderstanding or even disagreement on objectives, then there is trouble all round. Research people in industry should work on problems

which are written with the objectives stated and defined. It is at this point that the management and research come together on research objectives.

The research man is fully as interested in these research objectives as is the executive. The researcher wants to produce and produce something useful for the company with which he has thrown in his lot. It is a very unusual industrial research man who is not keenly interested in what the company wishes him to develop or to investigate. A lack of clear directives and uncertainty in company objectives can be very disconcerting. Nothing is so discouraging to the research man as to accomplish a mission, only to find that the result is not wanted. In the absence of clear directives, a good researcher will not sit back idly. He will go after what, to him, seems useful. If a man is working on the wrong project, is it his fault? The responsibility for research objectives belongs jointly to research management and to top management.

Futhermore, it would seem sensible to judge the research man's performance on the basis of how well he has carried out the scientific objectives of the problems to which he has been assigned. It is difficult to the point of impossibility for the business executive to judge the value to the company of the individual researcher. The executive has every right to demand long-term results from his research department, but the judgment of the performance of research individuals on essentially scientific objectives is a matter best left to the research director.

In industry the usual ladder of advancement is in administrative responsibility. To get ahead, one becomes a boss and then a boss's boss, etc. Administration and the administrative ladder exist in research, too, but a second parallel path of advancement has now been set up in most leading industrial laboratories. This is the professional ladder. It recognizes that the scientist can produce very substantial achievements, not only by working through people, but by individual, direct action. Based on demonstrated ability and accomplishment, a man can become, say, a

research associate or a senior research associate. These titles can have the same prestige and salary as a group leader or a section leader.

In addition to growth within the company, the researcher craves growth and recognition in the scientific community to various degrees. This recognition is attained by publication and by activity in scientific societies and meetings. In industry, strict limitations are placed on a man's freedom to publish and discuss his work. This is necessarily so, and it is largely accepted by the worker. He did accept it when he chose to enter industry rather than to stay with the university. Much information must be kept as a trade secret, or withheld until patents are issued. There is, however, research which is basic or concerned with methods, which it would be advantageous to the man and to the company to publish. Publication provides the man with satisfactions which are important to him. It helps the company by bringing the man into contact with other workers in his field and gains status in the professional world for the company's research. The latter has real usefulness, particularly in recruiting men. As a practical matter, for its own good, industry should have a policy of actively favoring publication. We should ask why should something be not published rather than why it should be published?

The matter of attendance at scientific meetings is a difficult one to administer. The psychological needs of men differ considerably. If a man never wants to leave the laboratory, he probably needs some stimulation. Another man may pursue, on the outside, a recognition which he feels is denied him within the company. To the best of our ability we should administer attendance at meetings as a useful part of research activity. To think of this attendance as a reward for good work or for longevity is a perversion. Scientific meetings are working sessions, not parties.

I wish that communication between the business man and the scientist were easier and more successful. The scientist has need of a special vocabulary and it is natural for him to talk in his own jargon. If he uses this in communicating with the businessman,

he is criticized. Yet the businessman has his own private language, whether he realizes it or not, and the effect of this language on the research man can be unhappy. The successful business executive is a master of persuasion and of the aggressive approach. He uses his imposing personality and he has, perhaps unconsciously, developed many devices whereby to sweep all before him. Before this virtuosity the researcher can feel helpless and defeated. He has not been trained in this skill. Actually, he is supposed to be not at all interested in winning arguments, but only in a search for fact through experiment and logic. The language barrier between the scientist and the business man is not all one way.

The effectiveness of the research individual and the collective performance of the group depend more importantly upon the spirit within the research department than upon contacts with the outside world. This spirit must be such that men communicate freely; that they are cooperative, generous with help and ideas; that they are more anxious to give credit than to claim it. Such a climate is created by the leadership in the department and by the way in which rewards in salary and advancement are given. The key is a responsive, encouraging research management. The physical side of the laboratory is of secondary importance. The scientist does not attach much value to fancy laboratories. In fact, he will tolerate quite primitive surroundings, if the other things which he values more are provided. I have a feeling that some of the more palatial industrial research laboratories have been built with as much thought to public relations as to the inhabitants.

At last we come around to money. Money is not the top motivating force of the typical research scientist. But this is not to say that the usual monetary incentives are to be ignored. Our research man is a scientist who has chosen a career in industry rather than in a university. In general, he has so chosen because of the higher pay and this means that he values money. Perhaps even more he values being treated fairly. A fair and competitive salary administration is essential, just as it is with everybody else.

I am not inclined to think that a bonus system based on

individual or company profits is a good thing for research people except for the few who are in higher administrative positions. Giving awards for patents we believe is a bad practice, tending to glorify artificial standards of contribution as defined by the U.S. Patent Office and to encourage secrecy and possessiveness of ideas. Cash payments for big accomplishments may also be dangerous and I think they are not very helpful in motivation. It is better to reward by salary and to administer this salary on a long-time basis, using standards of achievement which are based on a clear understanding of the research function.

The businessman and the industrial research scientist, although similar in many ways, are different in several important respects. The nature and point of view of each of these persons are valid and are appropriate to his role in the world. Each needs the other, and working effectively together is a practical matter of mutual understanding and respect. The research scientist is very strongly motivated towards his goals, which are to produce and to grow. Of first importance is to provide a climate which least inhibits this self-motivation. To the extent that this is done, I have no doubt that the research man will work long, hard, and effectively.